

***Interactive comment on “Effects of drought – altered seasonality and low rainfall – in net ecosystem carbon exchange of three contrasting Mediterranean ecosystems” by J. S. Pereira et al.***

**Anonymous Referee #4**

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General comments:

MS bgd-2007-0049 reports on the net ecosystem CO<sub>2</sub> exchange (NEE) measured by means of the eddy covariance method over three differing Mediterranean ecosystems in Portugal. The authors formulate two objectives: #1) to compare the NEE of these ecosystems which are typical for the area and #2) to compare the response of NEE to drought. The MS bears some novelty as it compares the NEE of typical Mediterranean landscape elements under similar climatic conditions. Otherwise the MS is relative standard in terms of methods used, presentation and discussion of results and falls fully within the scope of BG. However, there are several major issues in terms of the

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significance of their data and the structure of the MS (detailed below), which the authors, according to my opinion need to fix before the manuscript becomes acceptable for publication.

Specific comments:

(1) Objectives: The title and the conclusion section do not cover objective #1 - either the authors remove objective #1 or the find a more general title and modify the conclusion section accordingly.

(2) Uncertainty analysis: The basic requirement for a comparison of two or more sites is that any potential uncertainties are kept minimal and are thoroughly quantified. The manuscript is weak in both aspects. Different post-processing is applied at the three sites. At Tojal a 2d coordinate rotation is applied and no storage correction is applied (likely small, but would be easy to do). Furthermore, no frequency response corrections are applied to the open-path eddy covariance systems lead to an underestimation of fluxes (see Massman, 2000) and strictly speaking also the closed-path CO<sub>2</sub> flux should be corrected for density fluctuations caused by water vapour (Massman, 2004). These sources of systematic uncertainty could be minimised by appropriate post-processing. Another major source of systematic uncertainty is the choice of the  $u^*$  threshold. I suggest a full uncertainty analysis be conducted on the data quantifying both random and systematic uncertainties of NEE (e.g. Morgenstern et al., 2004).

(3) Soil water availability: Major parts of the MS deal with the effects of soil water availability on NEE, yet on no occasion data on soil water availability are shown. This is a major flaw of the MS and needs to be fixed - as far as I know soil water content measurements are part of the CarboEurope protocol and should thus be available at various depths at the sites. In order to compare the sites I suggest normalising soil water content measurements between field capacity and wilting point (i.e. use relative extractable water) in order to get rid of differences in soil water content caused by differences in soil physical properties of the sites.

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Detailed comments. (1) p. 1706, l. 9-13: express area in terms of percentage of e.g. non-built-up area instead of using absolute numbers

(2) p. 1707, l. 5: mention also ecosystem type when introducing site names

(3) p. 1707, l. 11-12: this is a repetition - remove

(4) p. 1708, l. 19-26: here it says that at all sites automatic weather stations were available but above it says meteo data were available only a two locations ?

(5) p. 1709, l. 7: NET carbon gain

(6) p. 1710, l. 6: give the corresponding correction factor for the Eugster-Senn attenuation model

(7) p. 1710, l. 27: explain classification into 0, 1 and 2 according to stationarity test (0-30% deviation, Ě.)

(8) p. 1711, l. 17: explain how LUE was calculated

(9) p. 1712, l. 10-19: remove the entire section - I do not see what Fig. 2 contributes to the objectives

(10) p. 1712, section 3.3: use other metrics to assess interannual variability, e.g. coefficient of variation; is inter-annual variability more due to GPP or Reco; do not compare sites in this section; how much of inter-annual variability is explained by precip and other environmental parameters

(11) p. 1714, section 3.4: here I would start by mentioning average NEE, GPP and Reco of the sites, maybe also ration of GPP/Reco

(12) p. 1714, l. 10: give  $r^2$ , p-value and equation of linear relationship - this may be useful for modelling

(13) p. 1714, l. 28: no grassland data are shown in Fig. 5

(14) p. 1715, l. 25: no other factor than precip was investigated !

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(15) p. 1716, l. 22: give range of values from Gilmanov

(16) p. 1716, l. 24-p. 1717, l. 4: no data are presented for this text, I thus would rather remove this section

(17) p. 1718, section 3.8: while an interesting effect, this section does not contribute to the objectives of the paper; in order to do so the authors would need to assess differences between sites and how important such pulses of CO<sub>2</sub> are for their carbon balance; if this is not done - remove

(18) Table 1: merge with Table 3

(19) Table 2: is redundant; precip can be shown in Fig. 1; other numbers can be mentioned in text

(20) Table 3: merge with Table 1; what do the footprint numbers refer to ? the extent of the homogenous area or some result of the footprint model (but for which conditions ?)

(21) Fig. 1: add annual precip

(22) Fig. 2: remove (see above)

(23) Fig. 3: bars should not overlap

(24) Fig. 5: data from Tojal are missing

(25) Fig. 10: remove (see above)

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